

# Questionable Arguments for the Correctness of Perturbation Theory in Non-Abelian Models

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## Abstract

We analyze the arguments put forward recently by Niedermayer et al in favor of the correctness of conventional perturbation theory in non-Abelian models and supposedly showing that our super-instanton counterexample was sick. We point out that within their own set of assumptions, the proof of Niedermayer et al regarding the correctness of perturbation theory is incorrect and provide a correct proof under more restrictive assumptions. We reply also to their claim that the S-matrix bootstrap approach of Balog et al supports the existence of asymptotic freedom in the  $O(3)$  model.

A recent paper by Niedermayer, Niedermaier and Weisz entitled "Questionable and Unquestionable in the Perturbation Theory of Non-Abelian Models" [1] purports to show that our criticism of the standard dogma regarding the alleged difference between Abelian and non-Abelian models is exaggerated and that there are good reasons to believe the orthodoxy. It is a positive development that members of the high energy physics community are now beginning to pay attention to the fact that this central issue for particle physics remains mathematically unresolved and that at least some arguments are needed in support of the conventional scenario. On the other hand, their paper also shows that even active workers in the field may fail to grasp fully some of the mathematical issues involved. Therefore we feel compelled to once again attempt to clarify where the troubles lie. In spite of the fact that our view of these matters differs from theirs, we appreciate their efforts to elucidate these important issues and deplore the lack of interest manifested by most particle and condensed matter physicists.

We begin by recalling that it is generally claimed, and repeated in the opening paragraph of [1], that the reason for the necessity of a non-perturbative definition of QCD is to study its non-perturbative properties, such as its spectrum. As we have been stating repeatedly [2], this is false: one needs a non-perturbative definition of a Quantum Field Theory because perturbation theory (PT) produces answers in the form of divergent (non-convergent) series. To interpret such series, and associate a numerical value with them, one needs a non-perturbative definition of the theory.

For theories like QCD and the two dimensional (2D) non-linear  $\sigma$  models the lattice version provides the needed non-perturbative framework. Many interesting questions, such as the spectrum, the relevance of PT, etc, can be asked and have well-defined, albeit sometimes unknown, answers. In particular, it has been assumed for years that if a PT computation is free of infrared (IR) divergences, then it must be 'right'. In the non-perturbative lattice framework, 'right' can be given a precise mathematical meaning: the difference between the non-perturbative (true) answer and the PT answer truncated at given order must be appropriately bounded:

$$|C(x, y; \beta, L) - \sum_{i=1}^k c_i(x, y; L) \beta^{-k}| = R_k(x, y; \beta, L) = o(\beta^{-k}) \quad (1)$$

Here  $C$  is some Green's function, say  $\langle s(x) \cdot s(y) \rangle$ ,  $x$  and  $y$  lattice coordinates,  $c_i(x, y; L)$  the PT coefficients for  $C$ ,  $\beta$  the inverse (bare) coupling,  $L$  the linear size of the lattice and  $R$  the remainder. The mathematical statement that PT is providing the correct asymptotic expansion of  $C$  in powers of  $1/\beta$  is nothing but a shorthand for the inequality (1). In many articles, conference presentations, etc one encounters the following meaningless statement: 'the PT series is asymptotic'. What is meant, is that the series is divergent. To

say that the series represents an asymptotic expansion makes sense *only* if a non-perturbative definition exists and inequality (1) can be verified.

For  $L$  fixed it is straightforward to prove (1). The subtle question is what happens when  $L$  goes to  $\infty$ ? In particular, in order to prove that taking the termwise limit  $L \rightarrow \infty$  in eq. (1) produces the correct asymptotic expansion of  $C$  one must control the remainder  $R$ , rather than merely prove that the limit  $L \rightarrow \infty$  of  $c_i(x, L)$  exists, as has been assumed for years in particle and condensed matter physics. In spite of vigorous attempts by mathematical physicists, this feat has been achieved so far only for Abelian cases [3], but Niedermayer et al claim to have found a new line of attack, which supposedly, if not rigorous, makes it entirely plausible that the same is true for the non-Abelian cases. Unfortunately, as we will argue next, we find that some of their arguments are mathematically imprecise and others outright incorrect.

Firstly, please note that there are two entirely different ways to let  $L$  go to  $\infty$ : i) independent of  $\beta$  and ii) as a given function of  $\beta$ . It could happen that the remainder  $R_k(x, y; \beta, L)$  is appropriately bounded in the latter case, but not in the former. In their paper Niedermayer et al ignore this obvious difference and essentially argue that they can control  $R_k(x, y; \beta, \infty)$  by controlling  $R_k(x, y; \beta, L(\beta))$  for an appropriately chosen  $L(\beta)$ , namely  $L(\beta) \propto \beta^k$ . Such a procedure is totally wrong: if PT is OK, then inequality (2) has to be obeyed at all orders for a given, ab initio made, choice of  $L(\beta)$ ; in other words, one cannot keep changing the function  $L(\beta)$  as one tests higher and higher orders of PT. Now if one makes the choice they recommend, namely  $L(\beta) \propto \beta^k$  for some given  $k$  and if the PT coefficients  $c_i(x, y; L)$  admit expansions in  $1/L$  (as they assume), then their choice for  $L(\beta)$  will change the whole expansion of  $C$  in powers of  $1/\beta$  (at sufficiently high order). In fact, for their choice of  $L(\beta) \propto \beta^k$  the PT expansion will depend upon the boundary conditions (b.c.) used, while the asymptotic expansion of  $C(x, y; \beta, \infty)$  is clearly independent of the b.c. used to reach the limit  $L \rightarrow \infty$ . Moreover if the finite volume corrections contained terms of the form  $\ln(L)/L^a$  with  $a$  some integer, as Niedermayer et al suggest, then their choice of  $L(\beta)$  would produce an asymptotic expansion containing also  $\ln(\beta)$ , an outcome which presumably they would not like for the infinite volume PT expansion.

A second point about which Niedermayer et al are imprecise concerns the distance  $|x - y|$  at which  $C$  is to be evaluated. Again, one could consider many cases, and the outcome could be vastly different. For instance one could consider the case  $|x - y|$  fixed (in lattice units),  $x$  and  $y$  at the center of the lattice,  $L$  going to  $\infty$ . This was the case analyzed by us analytically in [4]. It is not relevant for the continuum limit, hence our disagreement [5] with David's criticism [6]. For taking the latter limit, one must also let  $|x - y|$  diverge as a given function of  $L$  or  $\beta$  (see [7]). How PT would fare in these various possibilities is an open question. It is however clear that if it fails

at fixed lattice distance  $|x - y|$ , it will also fail for limits relevant for taking the continuum limit. Instead of stating precisely which limit they consider, Niedermayer et al make the vague statement (above their question Q1) that ‘all arguments are contained in a region whose size is negligible compared to the correlation length  $\xi(\beta)$ ’. They should state clearly if the distance  $|x - y|$  is fixed, or else, which function of  $L$  and/or  $\beta$  they consider.

Next let us consider the main strategy of Niedermayer et al for controlling the remainder  $R$ . It consists in assuming that correlation inequalities, proven for the Abelian case [8], also apply to the non-Abelian models. We have no objection to this hypothesis, which leads to their eq. (3.1):

$$C^{free}(x, y; \beta, \Lambda) \leq C^\infty(x, y; \beta) \leq C^{Dir}(x, y; \beta, \Lambda) \quad (2)$$

Essentially the whole argument of Niedermayer et al boils down to the fact that if the PT coefficients  $c_r^{free}(L)$  and  $c_r^{Dir}(L)$  converge to the same values for  $L \rightarrow \infty$ , then, because of inequality (2), so must the coefficients of  $C^\infty(x, y, \beta)$ . This is a priori false, as can be seen from the following counterexample: suppose that the true functions  $C^{free}(0, 1; \beta, L)$  and  $C^{Dir}(0, 1; \beta, L)$  were given by the following expressions:

$$C^{free}(0, 1; \beta, L) = 1 - 1/\beta(1 + 1/L) \quad (3)$$

$$C^{Dir}(0, 1; \beta, L) = 1 - 1/\beta(1 - 1/L) + 1/\beta^2 \exp(-\beta/L) \quad (4)$$

It can be easily verified that for any  $\beta$  and  $L$  sufficiently large, the following is true:

- $C^{Dir}(0, 1; \beta, L) > C^{free}(0, 1; \beta, L)$
- $C^{Dir}(0, 1; \beta, L)$  is increasing in  $\beta$  and decreasing in  $L$
- $C^{free}(0, 1; \beta, L)$  is increasing in both  $\beta$  and  $L$
- For  $L(\beta) \propto \beta$ , the remainder  $R_1^{Dir}(\beta, L(\beta))$  obeys their eq. (3.7b).

Consequently these expressions obey the conjectures made by Niedermayer et al. Nevertheless

$$C^{free}(0, 1; \beta, \infty) = 1 - 1/\beta \quad (5)$$

while

$$C^{Dir}(0, 1; \beta, \infty) = 1 - 1/\beta + 1/\beta^2 \quad (6)$$

Consequently, even if we accept their answer to their question Q2, the validity of correlation inequalities for non-Abelian models and their conjecture for the remainder eq. (3.7b), contrary to their claim, the answer to their question Q3 could be a resounding NO. Indeed, having ruled out the second part of their question Q3, let us add that the counterexample given above could easily be modified by taking the additional (non-perturbative) term to be of the form  $f(\beta) \exp(-\beta/L)$  with  $f(\beta)$  a decreasing function of  $\beta$  which does not possess an asymptotic expansion in  $1/\beta$  (such as  $\ln(\beta)/\beta^3$ ).

Before abandoning the discussion of this part of the paper by Niedermayer et al, let us point out two other clear errors related to their ‘heuristic proof that PT is OK’:

- 1) Below eq. (3.9) they state incorrectly that David proved the cancellation of IR divergences for the *remainder* of PT. This is false and in fact, such a feat would essentially amount to a proof that PT is correct. In fact, David’s proof for the cancellation of IR divergences in  $O(N)$  invariant Green’s functions was performed in the continuum, using a magnetic field IR regulator and dimensional regularization. We are not aware of any similar proof for a lattice model with any type of b.c.. There is also no proof that for  $L \rightarrow \infty$  free and Dirichlet b.c. give the same PT expansion, as they assume in the answer to their question Q2 (the statement may have been verified up to  $O(1/\beta^3)$  at best).
- 2) Their ‘heuristic argument’ in favor of eq. (3.9), is incomprehensible. While we see no connection between  $R_k^\alpha(\beta, L)$  in (3.9) and  $\bar{R}(\beta, \delta)$  in (3.12), even according to their own estimate the bound in eq. (3.14) does not look as that in eq.(3.9).

In conclusion, the claim of Niedermayer et al, that they have a heuristic proof that taking the termwise limit  $L \rightarrow \infty$  of PT with free and/or Dirichlet b.c. produces the correct infinite volume expansion of fixed lattice distance  $O(N)$  invariant Green’s functions, is unfounded. One could introduce an additional assumption and provide a rigorous proof. We will sketch the argument, even though we see no justification for these assumptions. Suppose that besides their assumption that PT expansions with free and Dirichlet b.c. converge to the same termwise limit for  $L \rightarrow \infty$  and that the convergence is like  $1/L$  (up to  $\log(L)$  powers) and that correlation inequalities are valid, we assume that the derivative of  $C^\alpha(x, y; \beta, L)$  with respect to  $L$  vanishes less fast than  $1/L^2$  for  $L \rightarrow \infty$  (as an example, please note that this would happen if in our counterexample we replaced  $\exp(-\beta/L)$  by  $\exp(-\beta/\ln(L))$ ). Now our counterexample does not work anymore because now, for  $\beta$  large but fixed, for  $L$  sufficiently large  $C^{Dir}(x, y; \beta, L)$  is increasing in  $L$ , in violation of the conjectured correlation inequalities. Therefore, if one could prove that:

- 1) Free and Dirichlet b.c. converge to the same limit as  $1/L$
  - 2) The remainder is bounded by  $1/\beta^k \exp(-\beta/\ln(L))$ ,
- then either the expansion of  $C(x, y; \beta, \infty)$  would be given by the (naive) PT or correlation inequalities would not hold for the model. For the  $O(2)$  model, presumably these assumptions are correct, however, as we discuss below, the IR divergences are expected to be milder.

Next let us discuss their claims regarding super-instantons (s.i.). Firstly, below their eq. (2.32) they claim incorrectly that in our paper [4] we stated that PT with s.i. b.c. produces IR finite answers. This is false and we never claimed anything like that: we showed only that at order  $1/\beta^2$  the answers are IR finite, yet different from those obtained with periodic b.c. *only* for non-

Abelian models. The latter point, regarding this manifest difference between Abelian and non-Abelian models, which we both verified and explained in our paper [4], is totally ignored by Niedermayer et al. In fact, if s.i. b.c. are ‘sick’, as they and David [6], would like to argue, how come they are alright for the  $O(2)$  model? Or are Niedermayer et al claiming that even for  $O(2)$ , the IR divergence they claim to have found at  $O(1/\beta^3)$  is present? This is an important point which they (and David) should address. It was similarly ignored by Brezin, David and Zinn-Justin (ref.20 in the Niedermayer et al paper) when they tried to argue that in  $1D$  the IR divergences occur ‘for dimensional reasons’: this is clearly false, since dimensional analysis works the same way for  $O(2)$  and for  $O(N)$   $N > 2$  models. Our explanation (see [4]) for this difference is that only for  $O(2)$  is the Gibbs measure a function of gradients, hence IR finite (to see this, parametrize the spin as  $(\cos(\phi(x)), \sin(\phi(x)))$ ).

Secondly, assuming that indeed PT with s.i. b.c. does become IR divergent at sufficiently large order, while with say periodic b.c. not, does it mean that taking the termwise limit  $L \rightarrow \infty$  of the latter produces the correct infinite volume expansion? The mathematical answer is clearly NO, since what is important for asymptoticity is control of the remainder, not merely finiteness of the terms. It should finally be remarked that it is even a stronger failure of the perturbative method if different b.c. not only give different results, but some give finite and others infinite answers. Since it is a priori not clear that the true infinite volume expectations actually have asymptotic expansions in invers powers of  $\beta$ , it is even conceivable that an infinite answer is correct in the sense that it shows the failure of such an expansion.

But the reason to doubt PT is more serious than the mere absence of a mathematical proof. What we have stressed over the years [9] [10] [11], is that PT is a saddle point expansion and for such a procedure to work, two conditions should be met:

- 1) the saddle should be ‘sharp’,
- 2) the saddle should be far from the edge of the integration region.

In  $O(N)$  models, on an infinite lattice, the Mermin-Wagner theorem guarantees that the saddle cannot be sharp. While this has been known for years, in our papers [4] [11] we showed that in the infinite volume limit super-instanton configurations become degenerate with the trivial vacuum; consequently *any* correct PT expansion, irrespective of the b.c. used, must include their contribution for  $L \rightarrow \infty$ .

So if in fact Niedermayer et al are right that PT with s.i. b.c. becomes IR divergent at  $O(1/\beta^3)$ , so does the correct infinite volume PT, whether the infinite volume is reached via free or Dirichlet or any other legitimate b.c., since a correct saddle point expansion should include expansions around many super-instanton configurations. From the double well harmonic oscillator it has been learned long ago that it is crucial to include such configurations that are nearly

degenerate with the ground state (in that case a gas of instantons and anti-instantons) in order to reproduce the correct asymptotics in the semiclassical limit (see for instance [12]). From the experience with that model one might expect that the effects of the super-instantons gas might be reproduced if one includes *all* saddle points, including the ones in the complexified spin space [13]. Niedermayer et al fail to appreciate this point. In their attempt to prove that s.i. b.c. are ‘sick’ they make another slip: namely their eq. (4.4) is wrong, the correct equation being:

$$\langle \mathcal{O} \rangle_{Dir} = \frac{\int d\mathbf{S}_{z_o} \langle \mathcal{O} \rangle_{S_{z_o}} e^{-\beta F(\mathbf{S}_{z_o})}}{\int d\mathbf{S}_{z_o} e^{-\beta F(\mathbf{S}_{z_o})}}. \quad (7)$$

But even more surprising is that they don’t seem to notice that their arguments for the ‘sickness’ of s.i.b.c. would equally well apply to the  $O(2)$  model, where in fact there is no difference between Dirichlet and s.i.b.c..

Before concluding, let us make another point regarding finiteness versus correctness of PT: in  $1D$  it is true that free b.c., which give a finite answer, give the correct answer. The reason is that in  $1D$  one knows the highest eigenvector of the transfer matrix (‘ground state’), which is just a constant on the sphere, and free b.c. only project onto that eigenvector, making the expectation values independent of  $L$ . No such simple  $L$  dependence occurs in  $2D$  with any b.c., hence there is no reason to make any analogy between finiteness and correctness with the  $1D$  case.

In the final paragraphs of their paper Niedermayer et al reiterate the standard non-perturbative arguments in favor of the standard dogma. We have answered many times these arguments, which we find wanting [10]. Let us briefly recap:

- In [14] we showed that in the  $1/N$  expansion the limits  $N \rightarrow \infty$  and  $\beta \rightarrow \infty$  do not necessarily commute for  $L = \infty$ .
- For  $O(3)$  the Bethe ansatz prediction for  $m/\Lambda$  [15] is larger than its MC value by about 15%. Of course, if our prediction that there is a transition to a massless phase at finite  $\beta$  is correct, then at some  $\beta$  the MC value for  $m/\Lambda$  must cross the predicted value, however with a non-vanishing slope.
- The MC data (produced by us) testing the bootstrap S-matrix prediction are not sufficiently well under control to say whether that prediction is correct (we find large lattice artefacts). This is the situation at low  $p/m$ . At large  $p/m$  both the lattice artefacts and the S-matrix prediction are under even poorer control, so to claim, as Niedermayer et al do, that the results coincide with renormalized PT and show asymptotic freedom is a gross exaggeration. In fact the MC data suggest that while the S-matrix prediction could be right, it most likely *disagrees with asymptotic freedom*: indeed, we find [16] that MC data for the dodecahedron model are indistinguishable from those for  $O(3)$ , but the former can surely not possess asymptotic freedom (since at sufficiently large  $\beta$  it possesses long range order).

While we find these arguments in favor of the accepted dogma wanting, we believe that our percolation arguments in favor of the existence of a massless phase in *all*  $O(N)$  models are much more compelling and under better theoretical control. Indeed in [17] and [18] we proved rigorously that for a different version of the  $O(N)$  models, the so called cut-action in which the spin gradient is restricted, either a certain well defined ‘equatorial cluster’ percolates or the model must be massless. Although five years have passed, no mathematical physicist has provided us, either in print or in private, with any heuristic arguments of how this equatorial cluster could possibly percolate. Moreover, as we stated in those papers [17] [18], if the equatorial cluster does not percolate, the typical configuration must be such that the inverse image of any sufficiently large piece of the sphere forms clusters of arbitrarily large size. As we emphasized in [4], such scale invariant configurations which we believe should be the typical configurations at low temperatures are very much like a gas of super-instantons, an independent observation, which came 3 years after the percolation arguments were written down. It appears to us that to close one’s eyes to these facts, as Niedermayer et al do, and simply reiterate the standard lore is not very scientific. If they find anything wrong with our percolation arguments, they should explain it; if not, they should worry that the standard picture may after all be wrong.

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